Does work impede child's learning? The case of Senegal *

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Abstract

This paper assesses the impact of labor performed during childhood on cognitive achievement of teenagers, measured by tests. Introduction of community fixed effects and use of multiple tests taken at the entry of primary school allows to control for unobserved heterogeneity and measurement error in the entry tests. We find no detrimental impact of participation of children to economic activities on their subsequent learning once controlling for the number of years of education but rather a positive, though small, impact. This could come from increased monetary resources or from knowledge acquisition due to a higher frequentation of the parents. Working more than 17 hours a week though prevents the child to learn as much as the other children.

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1 Introduction

In Senegal, one child out of three is working[1] while only half of the children receive a full primary education. Children are often portrayed as being either enrolled in school or economically active but not both in the same time. This representation is actually fallacious in most parts of the world and especially in Africa. In our Senegalese dataset, we find that 58% of working children are enrolled in school.[2] The picture is thus not as clear-cut as it may seem at first glance. Given the very low levels of education, understanding how they translate into human capital and how work may impede learning is valuable.

An important body of literature discusses the causes of child labor in developing countries; there is in comparison relatively little on the consequences of children's work on their future trajectories. While some people argue that every child should be entitled to live a work-free childhood, others consider child labor acceptable as long as it does not harm their development. The International Labor Organization's position is a mix of these two options since the Minimum Age Convention (C138) stipulates that:[3]

> National laws or regulations may permit the employment or work of persons 13 to 15 years of age on light work which is: (a) not likely to be harmful to their health or development; and (b) not such as to prejudice their attendance at school [...] or their capacity to benefit from the instruction received.

The aim of this paper is thus to assess the impact of children's participation to economic activities on their human capital accumulation. Research on this topic has so far mainly focused on the question of the impact of child labor on school attendance (Ravallion and Wodon, 2000; Boozer and Suri, 2001; Assaad et al., 2002; Canals-Cerda and Ridao-Cano, 2004; Beegle et al., 2004; Assaad et al., 2005)[4]. Most of these papers conclude to a fairly small effect of participating to economic activities on a range of educational outcomes related to enrollment.

However, in an environment where school quality is low, number of years spent in school may not be a very good indicator for future wages or productivity. Insofar as an important number of children are able to combine both schooling and work, the last aspect mentioned in the convention, namely the impact of participation to work on learning achievement, is particularly important and has seldom been studied. It could well be that labor has seldom any effects on the amount of schooling a child receives, while it has a detrimental impact on their learning. We thus prefer to analyze cognitive achievement of children rather than their education level.

Labor is generally expected to be detrimental to learning since it reduces time available for resting, attending school or studying at home. Two points

[1] Source: Unicef, wwww.unicef.org/infobycountry, following ILO definition of child labor.

[2] 15% of children combine both activities while 14% have none

[3] Children below 12 should not work, even if it is not harmful, while children older than 13 may be allowed to under the conditions stipulated in the text.

[4] Some authors also estimate the effect of having worked as a child on earnings in their adult's life (Emerson and Souza, 2008; Beegle et al., 2004).

need to be made readily: first, we will not address the question of how work disrupts schooling, meaning we will only measure the impact of work on learning for a given number of years of education. The only disruption that will be taken into account is the one on attendance for a child enrolled in school since it may well affect how much s/he gains from school. Second, this effect is potentially fairly small: in most cases, classes in primary school only last for half a day, leaving room for working during the other half. In addition, high agricultural season takes place during school holidays. It seems to be quite rare that children miss school in order to work. Still, exhaustion and impossibility to do homework could significantly slow down learning. On the other hand, working increases the available income and allows to pay for some expenses associated to education: transportation, lunches outside home, books or even tutorials. Moreover, working along with parents (since it is the prevalent working environment for children in Africa) may be an opportunity to learn from them. The sign of the effect of work on learning is thus ambiguous and needs to be empirically estimated.

To my knowledge, the only paper that estimates the impact of child labor on learning achievement measured by tests is Heady (2003) on Ghana.[5] He finds that work has a substantial negative effect on learning achievement in reading and mathematics if it is performed outside the home (even when controlling for education level of the children). Several drawbacks need to be considered though in this paper: first, it only uses current information of children's time allocation while the process of human capital accumulation is a long-term one. Current status of the child regarding school and work is probably a poor proxy of the past time allocation decisions especially since these choices depend highly on age. Second, human capital investments are likely to be affected by characteristics such as cognitive ability, preferences, school quality, working environment which may also determine learning achievement. While Heady attempts to control for these characteristics with a Raven's matrix, the strategy has some limitations: Raven's matrices may not only capture innate cognitive ability but also be affected by education as other tests scores have demonstrated to be (see, for example, Hansen et al. (2004)). This is even the more so that these tests results have been collected at the same time than the other outcomes. In addition, using this variable in a control strategy assumes there is no measurement error in abilities. Lastly, while it is argued to capture heterogeneity in innate abilities, other environmental factors such as school quality or labor market organizations are unlikely to be captured by such a variable and, as such, is unobserved heterogeneity that could affect both investments decisions and results.

As a consequence, our paper tries to investigate a bit further the relationship between participation to economic activities and learning achievement. To address the previously mentioned caveats, we use the EBMS data which pro-

[5]Other papers that incidentally address the question by using other indicators of achievement include Patrinos and Psacharopoulos (1995) (grade repetition) and Akabayashi and Psacharopoulos (1999) (parental assessment of the ability of the child to read and do written calculations). Both of them find virtually no effect of labor on cognitive achievement.

vides tests results for a sample of Senegalese teenagers along with retrospective information on work and schooling. The number of years spent working and at school should be more relevant for explaining children's learning achievement than their current status. In order to take into account the endogeneity issue for time allocation decisions, we combine the following strategies: we introduce communities' fixed effects to control for heterogeneity such as differences in school quality and working opportunities on the one hand and we control for results obtained to tests taken at the entry of primary school on the other hand. These tests can be argued to capture true innate ability given the early age at which they are taken. Nevertheless, they still could be spoilt by measurement error. Availability of other tests taken by the same pupils later in the year allows us to deal with this issue and our strategy provides reliable estimates of causal impact of work on learning achievement.

We find that work is not detrimental to learning achievement per se: once controlling for education level, the effect of one additional year of working is significantly positive for 3 tests out of 4. Working more than 17 hours a week though depletes accumulation of human capital. On a more methodological aspect, controlling for selection bias through measures of ability and communities fixed effects proves important and indicates that the naive estimate of the effect of work is downwardly biased, as expected.

Section 2 describes data and provides some statistics on the relationships between working and schooling decisions. Section 3 presents the empirical model and section 4 offers the results.

2 Data description and time allocation of children in Senegal

2.1 EBMS and PASEC datasets

The data used in this paper comes from an original survey entitled Éducation et Bien-Être des Ménages au Sénégal (EBMS)[6] conducted between April and June 2003 and to which the author of this paper took part. This survey covers a national sample of 1800 households. It provides information on household composition, household asset ownership and housing characteristics. At the individual level, information was collected on education, health, employment status and activities of every household member. We thus know the detail of children's schooling trajectories and their working starting date.

The sample includes households of children who participated in an earlier survey conducted by the PASEC.[7] These children, who were roughly aged between 7 and 10 in 1995, were tested every year as long as they were still enrolled.

[6]This survey was designed by a team composed of Peter Glick, David Sahn, and Léopold Sarr (Cornell University, USA), and Christelle Dumas and Sylvie Lambert (LEA-INRA, France), and implemented in association with the Centre de Recherche en Économie Appliquée (Dakar, Senegal).

[7]Programme d'analyse des systèmes éducatifs des pays membres de la CONFEMEN.

This paper only uses the first tests in mathematics and French, which took place at the beginning (in 95) and at the end (in 96) of their second grade. The EBMS survey was designed so as to complement the PASEC survey, in order to obtain more information on the households.[8] The cluster structure of the original PASEC survey was therefore maintained. We recovered on average 13 children per cluster (out of the 20 who participated) for 60 clusters from the original PASEC sample.

Moreover, a sub-sample of teenagers took several tests designed by the INEADE:[9] two of these tests (life skills and easy mathematics) are oral ones, while the two more advanced (French and advanced mathematics) are written. In addition, the oral ones could be taken in local language rather than French if the child wished so.[10] In total, 2365 children have taken at least one out of the four tests in 2003, 1597 have taken the four and among the PASEC children, they are respectively 641 and 497. The core sample for this paper is constituted of PASEC children tested in 2003. We only keep children aged between 13 and 18 at the time of the EBMS survey, in order to limit measurement error and to insure comparability between the children.

2.2 Key variables

As previously mentioned, we aim to assess the impact of child work on learning achievement. Learning achievement will be measured by tests taken in 2003 by teenagers while their initial abilities are assessed by tests they have taken in 1995. In order to take into account the full work trajectory, we use the number of years where the individual has performed an economic activity during childhood. Controlling for time spent at school allows us to evaluate the pure effect of work on learning. To do so, we compute the number of years spent studying. This section gives more details on how we define these variables and briefly discusses the control variables introduced in the estimations.

2.2.1 Education and work

The number of years of education is computed by adding the number of repetitions to the last grade completed by the pupil. Given that the survey took place just before the end of the school year, children attending school at that time had nearly completed one more year than the last passed grade. For those children, we add one to the previous calculation. The resulting variable is thus the number of years of education at the time of the survey, independently of the final grade attained. If child labor increases repetition then it will show up in the estimates since for a given number of years of education, working children

[8] Another goal of the EBMS survey was to increase the sample with households not sending their children to school in each cluster. This aspect is not used in this study.

[9] Institut National d'Étude et d'Action pour le Développement de l'Éducation. The INEADE had already been in charge of designing the tests for the PASEC.

[10] French is very rarely spoken in the families and is learnt at school.

would presumably have attained lower grades than the ones who do not work and as such would have lower outcomes measured by tests in 2003.

In the data set, we do know the age when the child started working but we do not know if s/he has been working continually since. What we call "number of years of work" is actually the number of years since the child started working. Consistently with the ILO definition of work, the study only takes into account participation to economic activities. The rationale for focusing on work duration is that even if the child interrupted his/her activity, it should still be a good proxy for past time allocations: children who already have performed an activity are more likely to do it again. Actually, only 3.24% of children who have ever worked have not been performing any economic activity during the year of the survey. In addition, working very early in life may be particularly detrimental and this should be captured when considering age when started working. In addition of age, we have some information on the working conditions when the child started, namely hours of work and employment status (employed, independent or familial worker). In the last section, we enquire into whether these various measures of work intensity matter for learning.

Participation to domestic work though could also have detrimental effects on learning since it is time consuming. In addition, it is rarely seen as income generating not allowing the child to acquire specific human capital unlike economic activities. The lack of retrospective information in EBMS for the participation of children in housekeeping prevents us from studying such a mechanism, and in any case it would have been very prone to measurement error. and would in any case be very prone to measurement error. Some information on the child and his/her household was actually collected by the PASEC through an auto-administered survey but given the early age of the children and the fact that the questions were in French, the answers are obviously spoilt with measurement error. Among other things, children had to stipulate what domestic chores they were performing. We use this information for robustness checks but do not endeavor to estimate the impact of participation in housekeeping on subsequent learning outcomes, and will only provide limited evidence on the question.

2.2.2 Tests

Tests in 1995 (and subsequent years) and then in 2003 have all been designed by the INEADE. In 1995, there were only two written tests, one of mathematics, the second of French; in 2003, four tests are available: easy and advanced mathematics, French and life skills. They aim to assess cognitive acquisition due to school. As such, they are focused on evaluation of schooling outcomes such as reading, vocabulary, conjugation for French tests and additions, subtractions, reading time, measures assessments, small problems for mathematics. Life skills test is somewhat different: it aims to assess knowledge of the child on different common topics, with a focus on hygiene (water purification, diarrhea, malaria, etc.). For most parts, tests take a multiple-choice questions format. Children who do not answer to some questions but take the test are granted the points corresponding to a random answer (e.g., if they have to choose between three answers, they get one-third of the points). This is equivalent to the standard procedure of giving negative points to the ones who provided a wrong answer. Tests are marked from 0 to 100. For results measured in 2003, we normalize scores in order to interpret and compare coefficients: we divide the result by the standard-error of the score on the population.[11]

All the Pasec children were attending the second grade of primary school in 1995-96. Since school is compulsory from age 7, they all should have been aged 8 or 9 at that time. There was in fact considerable variance. First, a child could have started school before (after) the legal age and thus be younger (resp. older). Second, he may already have repeated the first or second grade and thus be older. Children may perform differently at tests in 1995-96 depending on their age or, to put it differently, assessment of their innate abilities must take into account this heterogeneity. For convenience, we use age in 2003 and given that it is a linear function of age in 95, the estimate captures the age differences when in second grade. Older children in 1995 have in fact lower abilities for a given test level and as such are likely to perform worse at tests taken when teenagers. We thus do expect a negative correlation between age and outcomes in 2003.

2.3 Sample and attrition

As previously mentioned, we wish to use observations for which both tests taken in 2003 and tests taken in 1995 are available. This raises the question of attrition since not all Pasec children were tested again. Possible reasons for not having been re-interviewed are the following:

- child's household has not been found, generally because it has moved;
- the household has been found back but the child has not been tested. This could either be due to the fact that the child does not live anymore in the household or that he was not available for the tests.[12]

Among the 1054 Pasec children who have taken the two entry tests and living in the 60 areas surveyed in the Ebms[13],

- 846 (80%) belonged to a reinterviewed household;
- among which 594 (70% of the 846) have been tested.

In the end, only 56% of the original sample is tested again. This high attrition rate needs to be addressed. The Pasec survey includes a small questionnaire which helps for describing the different attrition mechanisms since it

[11] Moments for the normalized test scores are reported in Table 9.

[12] Given that we have adopted a fairly loose definition for the household and specifically on whether a child belongs to it in order to increase the registered information on children away from the household a large part of the year, we do not address this distinction.

[13] 1130 of them have taken at least one the two entry tests (3 areas have only started their tests in 1996).

is available for the full sample.[14] We first explain whether a household has been found back and then, for children whose household has been identified, whether they have taken at least one test. Results are provided in Table 15 in appendix.

The main results are the following: living in a rural area, with one's mother or grand-parents and in a poor household increase the likelihood of finding the household. When the child has been doing some agricultural work, presumably when the household owned land, the household is more likely to be re-interviewed. For the matter of individual attrition in a re-interviewed household, results are the following: children from wealthier households or those who have not repeated their second grade are more likely to be successfully tracked. This may be due to the fact that some households choose to send their succeeding children away to attend lower secondary school. Scores in second grade and children's activities though have no effect on attrition.

Given that we suspect this first survey to be very imprecise, we do not endeavor to take selection into account in our estimation. The strategy offered hereafter, namely controlling for usually unobserved characteristics, will clearly mitigate this issue but the conservative claim is that the results are only representative of the sample of children who have stayed in their household, which itself has not moved.

2.4 Children's education and labor in Ebms data

This section briefly describes the distribution of the previous variables for the sample of interest. Among Pasec children[15], aged 13 to 18, half of them have more than 9 years of education at the time of the survey (recall that it is time spent enrolled at school: half of children have only completed grade 6) and the average number of years of education is 8. The average number of worked years is 2.46, while the median is 0. Consistently with the possible combination of both activities, the correlation between the two variables is -0.14 and is statistically significant at the 5% level.

Two measures of work intensity are available: number of hours when the child started working and the average between this and the work intensity at the time of the survey. It turns out that the correlation between the two is quite high (0.9) and that their means reflect the fact that work intensity tend to increase with age (see Table 1). In the last part of the paper, we thus only use the number of hours when the child started working. The average number of hours at the beginning (0 if the child has never worked) is 5.9 hours per week but a working child performs on average 14.2 hours per week. Half of working children were working less than 10 hours per week though when they started their activity. These figures probably hide considerable discrepancies between different times of the year due to the agricultural cycle.

[14] We are reluctant to make an intensive use of these variables since they are reported by young children and are thus very prone to measurement error.

[15] We actually compute the statistics only for children who have effectively taken the entry tests.

Table 1: Annualized hours of work per week

		Whole sample	Working Children
Hours at beginning	Average	5.9	14.2
	Median	0	9.7
	# Obs.	552	230
Average hours	Average	6.4	15.0
	Median	0	8.7
	# Obs.	538	230

Note: Sample constituted of Pasec children between 13 and 18 years old, who have taken at leats one test in 95 and the oral mathematics test.

Table 2: Employment status of children when they started their activity

	Urban	Rural	Total
Employed	17.9%	2.6%	6.2%
Independant	4.8%	4.4%	4.5%
Familial worker	40.5%	88.1%	76.8%
Apprentice	36.9%	4.8%	12.5%
# Obs.	84	269	353

On the matter of the type of work, Table 2 reports the employment status of children (again when they started working), by type of area. Family worker is by far the most important category and is more prevalent in rural areas than in urban ones. Employed children and apprentices are on the contrary mainly found in urban zones. We should be cautious though when using this variable because of the low number of observations for each category.

Let us now turn onto the association between work and low results. We compare means between children who have worked and those who have not. Results are consigned in Table 3.[16] The results are consistent throughout the tests offered to the children. Those who have worked during their childhood have systematically lower results when they are teenagers; the difference varies from 3 to 6 points out of 100 and is always significant at the 10% level. This conforms to the general view that work is detrimental to human capital accumulation. On the right panel of the Table, we run the same exercise by comparing children

[16]For consistency purposes, we provide the results for the same sample. With the larger sample of children who have taken the tests in 2003, we get very similar results but the mean differences are systematically significant at the 1% level.

Table 3: Past activities and mean test scores in 2003

Test	No work	Some work	p-value	Less than 8 years of edu	8 years or more	p-value
Life skills	66.81 (330)	59.96 (284)	0.00	58.32 (212)	66.09 (411)	0.00
Oral maths	62.42 (327)	59.38 (271)	0.08	54.87 (205)	64.00 (401)	0.00
Written maths	77.62 (294)	73.86 (202)	0.04	64.03 (114)	79.25 (366)	0.00
Written French	80.45 (292)	77.07 (205)	0.05	69.33 (116)	81.71 (385)	0.00

Note: Sample constituted of Pasec children between 13 and 18 years old, who have taken at least one test in 95 and the test of interest; the number of observations for each mean is in parenthesis below the figure. "No work" means that the child declares no work during his/her childhood. The p-values give the statistical significance when testing the difference between no work and some work for the third column and between children who have attended school for more than 8 years and those who have not for the sixth one.

who have attended school for 5 years or more and children who failed to do so; as expected, more educated children get better scores and the difference is quite important (around 8 points). The next section explains why this raw correlation may not be very informative on the real impact of child work on his/her cognitive achievement and how we can proceed to exhibit such an effect.

3 Estimation of a human capital production function

The aim of the paper is to assess the impact of work on human capital accumulation. This can be formalized in the following way:

$$T_{ic} = F(W_{ic}, S_{ic}, \delta_c, X_{ic}, u_{ic}) \quad (1)$$

where T_{ic} is a measure of cognitive achievement for child i living in community c, W_{ic} and S_{ic} respectively number of years spent working and enrolled in school, δ_c quality of education and working opportunities in community c, X_{ic} personal characteristics such as gender, age and socioeconomic background of child i and u that may reflect any unobservable determinant of results (preferences, innate abilities) or measurement error. We concentrate on the estimation of a linear relationship between cognitive outcomes and child's investments in human capital since specification tests do not detect any nonlinear form of relationship. S_{ic} is expected to have a positive sign, while sign for W_{ic} has to be determined empirically as discussed in introduction.

3.1 Issues to tackle in the estimation

The crucial point of our paper is to note that time allocation decisions (between work, schooling and leisure) are likely to be endogenous in equation (1). This could either come from the presence in u_{ic} of specific abilities of the child or from failure to control for all the relevant characteristics of the environment.

As for specific capacities of the child, more able pupils may spend more time in school/devote more energy and thus get better scores; the opposite may happen if schooling time and innate abilities are somewhat substitutes rather than complements in the human capital production function. In the same vein, children with lower endowments for education may also devote more time to work in order to loosen budget constraints and permit siblings' schooling. The outcome of the allocation decision depends on human capital production function and on parents' preferences for equity as discussed by Becker and Tomes (1976) but in any case, we do expect to see a correlation between time allocation decisions and unobservable heterogeneity such as innate abilities.

Moreover, living environment of the children affect both their choices and their cognitive achievements. Indeed, one unobserved factor in the human capital production function is the effort made by the pupil to learn. This effort is surely correlated with what s/he expects to gain from learning. Good schools, high returns to education in the labor market will affect both how much education they demand and how much they strive to benefit from it. On labor's side, places with working opportunities for the children might be places with low returns to education. This is notably the case of agricultural areas, where most of child labor is found and where skilled jobs are rare. If this were to be true, we would expect to find a downward bias when estimating the impact of labor on learning.

Lastly, some household's characteristics may both determine children's time allocation and have an effect on their cognitive achievement. Parental education , productive assets detention or gender, for instance, are likely to change households' expectations of returns to education and, as such, may affect the effort devoted to learning.

For all these reasons, W_{ic} and S_{ic} are likely to be correlated with u_{ic}. The aim of the paper is thus to provide an estimation of the effect of one additional year of work on cognitive achievement of children, net of spurious correlations.

3.2 Empirical strategy for identification

The empirical strategy we propose is a control one. This is motivated by the fact that standard options such as finding appropriate instruments or relying on natural experiments are very difficult to implement in this setting. A convincing instrument could be a ban of child labor or a change in compulsory schooling laws but none of it exists in Senegal nor could be enforced in most developing countries. Moreover, using a control strategy will allow us to try diverse specifications, which would not be the case if only a reduced set of instruments was to be found.

Let us now detail our identification strategy. It is quite obvious from the previous discussion that two sources of endogeneity coexist. The first one lies in unobserved environmental variables and common to all children of the area while the second lies in choices specific to children, either due to their abilities or to their (or their parents') preferences towards education.

3.2.1 Communities fixed effect

Since households are clustered in 60 communities, we are in the position to control for environment differences by introducing communities fixed effects. These fixed effects will capture school supply and its quality, but also working opportunities and social norms prevailing in the area. Fixed effects are particularly useful for the last two aspects since social norms and expectations for returns to education and work are very difficult to observe.

3.2.2 Results to tests taken at the entry of primary school

Controlling for heterogeneity in abilities of the individuals is a much more difficult task. Since it is a recurrent issue in the returns to education literature, there is already a fair amount of contributions to the question (Griliches, 1977). Results to tests taken at the entry of primary school can be considered as proxies for innate abilities. They are measured after only one year of schooling and probably reflect intrinsic capacities of the children as well as the learning environment in which they have lived when infant. Controlling for innate abilities of the children this way should leave in the residual term only measurement error and other "safe" unobserved determinants. However, failure to capture the whole heterogeneity (also called "measurement error" in the indicator) would lead to biased estimates, as shown by Hansen et al. (2004). We thus chose to exploit the repetition of tests within the same year in order to implement the multiple indicator solution offered in Wooldridge (2002). If two indicators of the same unobserved variable (here, innate ability) are available, we can combine both of them to get an estimate of the true innate ability indicator, even if both are measured with error. This is done by instrumenting one of the indicators by the other and allows us to consider that we control for the true value of innate abilities. More specifically, if *abil* stands for the true innate abilities, we assume that:

$$\begin{aligned} T^{early\_1} &= \delta_0 + \delta_1 abil + a_1 \\ T^{early\_2} &= \rho_0 + \rho_1 abil + a_2 \end{aligned}$$

where T^{early_1} and T^{early_2} are two proxies for $abil$ and a_1 uncorrelated with explanative variables of the interest regression, $\delta_1 \neq 0$, $Cov(abil, a_1) = 0$ and the same for the second equation. In addition, a crucial assumption is the absence of common measurement error in the two indicators or, to put it differently, that the correlation between the two indicators arises only from their common dependence on the true indicator ($Cov(a_1, a_2) = 0$).

To sum up, we wish to use tests taken at the entry of primary school to control for innate abilities of the children when explaining cognitive level in 2003, but also exploit the fact that they are several to take into account the measurement error imbedded in each of them. We actually have two options for implementing this approach. Two tests are available for each session when the child was enrolled in second grade of primary school: French and mathematics. We can thus either use:

- only tests taken in the same discipline, one being measured at the beginning and the other at the end of second grade ($T^{95\text{-}F}$ and $T^{96\text{-}F}$ when trying to describe outcomes in French in 2003 for instance) or
- only tests taken at the entry ($T^{95\text{-}F}$ and $T^{95\text{-}m}$) whatever the outcome.

Both strategies have their pros and cons that we detail later on.

Let us start with some common considerations. All of these early tests are very likely to capture the same information on schooling abilities at a very young age, which can be seen as the latent variable. These tests were designed so as to evaluate the progression of children through school. Cognitive achievement is then measured by similar tests, designed by the same institute, which had received instructions to produce comparable tests except that they were for a higher level. As a consequence, the initial score as well as the final ones are focused on the same specific abilities of the children. It is thus expected that children who are faring well with the tests when they are in second grade are also the ones who do better later on. For this reason, the indicators can reasonably be argued as capturing the relevant information: they may not do justice to all the dimensions of human capital but it would not be the case neither of the final cognitive achievement indicator. Note however that the life skills test differs from the other tests and that this argument may be less accurate in this case.

The first option consists in using only results obtained in French (both at entry and end of second grade) when we explain French outcomes in 2003 and results obtained in maths when we explain math results in 2003. This strategy is legitimate if the relevant ability variable is specific to the discipline and well predicted by two successive tests in the same discipline(for life skills, given that it is not obvious to guess which latent variable is the most likely to affect final outcomes, we try both). The main caveat of this approach is that the way children have progressed along the year may be affected by their learning effort. Another way to put it is that results measured at the end of the second grade are more likely to be spoilt with other factors than true innate ability, as for instance, capacity to learn and make progress, and that we do not fully control.

For these reasons, it may be worth focusing on abilities measured at the entry of second grade. In this case, our last point does not apply; unfortunately, this also implies that the tests may not capture information on abilities of the children to progress. This being said, the main caveat is that if we consider that abilities for the children in terms of French and of mathematics are very different and do not proxy for the same latent variable of capacities, then the approach is not valid.

To conclude on the comparison between the methods, it is worth noting that the data could give us some hints about what the relevant latent variables are through correlations between indicators (a strong correlation between two indicators reflect the fact that they tend to describe the same latent variable); unfortunately, Table 10 in appendix shows it is not the case and confirms that these indicators are prone to measurement error and that the multiple-indicator solution needs to be implemented.

Most of the criticisms (in the returns to education literature) of this technique lied in the fact that tests that were used were collected at the end of the education span and thus could have been affected by it; in our case, tests used as controls cannot demonstrate a reverse causality issue since they are collected when the child has hardly started its education. This strategy provides estimates of the impact of one additional schooling or working year on cognitive achievement for a given level of endowments. However, the latent variable of ability that we try to measure indirectly via tests is made of different components: true innate ability but also parental preferences for school versus work that may have affected child's cognitive achievement even with an early measure in second grade (Dumas and Lambert, 2007). We thus have to assume that the mix of these ingredients does not change greatly over the school life course of the child and affects in the same way his/her outcomes when teenager.

3.2.3 Household's characteristics

As we mentioned, growing environment of the child, as well as characteristics that may correlate with expected returns to education, can determine both time allocation decisions and learning effort and hence cognitive achievement. Some of these differences may be captured by early test scores but there may remain some heterogeneity. In a second stage, we thus introduce controls for parental education, gender, number and composition by gender of the siblings, household's size, possession of productive assets, wealth and check whether the results change. This should be indicative of whether there remain some correlation between u and S and W. Appendix 11 describe these variables.

3.2.4 In practice

The estimation method implied by these considerations is actually quite simple: it consists in running an IV estimation on

$$T_{ic} = K_0 + \alpha W_{ic} + \beta S_{ic} + \gamma X_{ic} + \sum_{c=1}^{60} \delta_c + \eta T_{ic}^{early\_1} + u_{ic}$$

where T_{ic} stands for the various outcome variables (life skills, oral maths, written maths and written French tests) and $T_{ic}^{early\_1}$ is instrumented by $T_{ic}^{early\_2}$.

The previous discussion suggests different options for the choice of T^{early_1} and T^{early_2}, which can be freely exchanged; we sum up these options in Table 4.

Table 4: Assumptions and control tests in the regressions

	Assumptions	Outcome test	T^{early_1}	T^{early_2}
Option 1	latent variable may be specific to discipline;	LS, WF	T^{French_95}	T^{French_96}
	ability to progress taken into account	LS, OM, WM	T^{maths_95}	T^{maths_96}
Option 2	maths and French tests reflect the same ability; ability to progress not taken into account	LS, OM, WM, WF	T^{maths_95}	T^{French_95}

Note: LS stands for life skills, OM for oral maths, WM for written maths and WF for written French.

Depending on the implemented strategy, we need for each observation the tests results obtained in 2003 and some scores in mathematics and French obtained at the beginning and at the end of school year 1995-1996. In appendix, the reader will find the number of available observations for each test (Table 13) and descriptive statistics of the outcomes (tests results in 2003) for various subsamples (Table 14).

The lower number of observations for written tests is due to the fact that children needed to be able to write in order to take that test. Given that we control for unobserved abilities, selection bias should not be an issue. Moreover, Table 14 clearly shows that restriction to children having taken the tests at school entry is unlikely to bias the sample since they have fairly similar outcomes.

4 Results

4.1 Impact of time devoted to work and education on cognitive achievement

We discuss in detail the results based on a range of specifications, going from OLS to the full model (communities fixed effects and early scores instrumented). Tables 5 and 6 only report estimates of interest, namely effect of one additional year of work/school respectively. For sake of simplicity, option 1 has been retained for discussing the detail and results for option 2 are only delivered in the last columns for comparison.

We first discuss the estimated impact of child labor on cognitive achievement (Table 5). It appears at first glance that coefficients are only negative for life skills, but never statistically significant in the final specification. Estimates of the impact of child labor are positive for mathematics results, be they basic or

Table 5: Estimated impact of child labor

Scores in 2003	1	2	3	4	5	6	7
Life skills	-0.035**	-0.034**	-0.012	-0.011	-0.008	0.001	-0.008
	(0.013)	(0.013)	(0.016)	(0.016)	(0.017)	(0.018)	(0.017)
Oral maths	-0.001	0.000	0.033+	0.034*	0.032+	0.044*	0.035+
	(0.013)	(0.013)	(0.017)	(0.017)	(0.017)	(0.019)	(0.018)
Written maths	0.009	0.011	0.034*	0.032+	0.029	0.033+	0.033+
	(0.014)	(0.014)	(0.017)	(0.017)	(0.018)	(0.019)	(0.018)
Written French	0.011	0.014	0.036+	0.034+	0.026	0.032	0.032+
	(0.015)	(0.015)	(0.019)	(0.019)	(0.017)	(0.021)	(0.019)
Fixed effects	no	no	yes	yes	no	yes	yes
Scores in 95	no	yes	no	yes			
Scores in 95 instrumented by 96					yes	yes	
Math scores in 95 instrumented by French score in 95							yes

Note: Scores are normalized by their standard errors thus estimates are the impact of one additional year of labor on scores in 2003 measured in standard errors. See Table 13 for the number of observations. Control tests for specifications (2) and (4) to (6) are French in 95 for written French and mathematics for all the other ones. **, * and + mean respectively that the coefficient is significantly different from 0 at the 1%, 5% and 10% level.

advanced. We thus have little empirical support for a detrimental impact of child labor on learning, controlling for time spent enrolled in school. Second, if we compare specifications (1) and (3), we find that controlling for village fixed effects leads to a higher positive impact of child labor on knowledge, or, to put it differently, that controlling for village fixed effects corrects for a negative bias in the estimates of child labor. This is consistent with the argument that areas with child labor are also the ones where individuals invest less in learning. Third, when comparing specification (1) and (2), we observe that controlling for abilities has no effect on the estimates. Fourth, introducing abilities when we are controlling for fixed effects (switching from estimation (3) to (4)) has also little effect. However, for reasons detailed before, this estimation may still be biased. Instrumentation of scores (from estimation (2) to (5) or (4) to (6)) tend to increase the estimated effect of child labor on tests, showing that in most of the cases not taking into account the measurement error imbedded in tests will bias the results. This is consistent with the fact that less skilled children are more often put to economic participation than the others. This being said, the change in the estimates is rarely significant at the 5% level. The most convincing estimation is thus the one provided in column (6) where we control

Table 6: Estimated impact of school attendance

Scores in 2003	1	2	3	4	5	6	7
Life skills	0.124**	0.118**	0.114**	0.106**	0.077**	0.071**	0.101**
	(0.020)	(0.020)	(0.019)	(0.019)	(0.028)	(0.024)	(0.020)
Oral maths	0.122**	0.119**	0.138**	0.131**	0.075**	0.107**	0.122**
	(0.020)	(0.020)	(0.020)	(0.020)	(0.028)	(0.024)	(0.022)
Written maths	0.200**	0.196**	0.219**	0.216**	0.180**	0.207**	0.212**
	(0.025)	(0.025)	(0.024)	(0.024)	(0.033)	(0.028)	(0.025)
Written French	0.180**	0.173**	0.177**	0.171**	0.140**	0.137**	0.162**
	(0.027)	(0.027)	(0.026)	(0.026)	(0.032)	(0.032)	(0.027)
Fixed effects	no	no	yes	yes	no	yes	yes
Scores in 95	no	yes	no	yes			
Scores in 95 instrumented by 96					yes	yes	
Math scores in 95 instrumented by French score in 95							yes

Note: Scores are normalized by their standard errors thus estimates are the impact of one additional year of labor on scores in 2003 measured in standard errors. See Table 13 for the number of observations. Control tests for specifications (2) and (4) to (6) are French in 95 for written French and mathematics for all the other ones. **, * and + mean respectively that the coefficient is significantly different from 0 at the 1%, 5% and 10% level.

for communities fixed effects and instrument the score: we find that child labor has no impact on life skills test and a positive impact on maths, larger on basic skills than the advanced ones. The estimated effect on written French is of the same order of magnitude than for the written maths but fails to be significant, even at the 10% level. Specification (7), where the second option is implemented, confirms the results and identifies a positive effect of time devoted to labor on cognitive achievement that is significant at the 10% level for all tests except the one on life skills.

Table 6 indicates that an additional year of schooling leads to a significant increase in cognitive achievement: the estimated impact varies roughly between one-fifteenth and one-fifth of a standard error and is thus higher than the impact of one year spent working. Controlling for scores leads to small variations in the estimated effect for most of the tests. If we use the instrumented scores, we then get lower estimates. The biases generated by individual heterogeneity were thus positive: this seems to show that parents do not compensate for unequal capacities of their offspring. Controlling for village fixed effects does not correct for biases of the same sign depending on the tests. Again, specification (7) is in keeping with that has been found with the first option. The estimated effect of education is much smaller on life skills than on the other topics, while the written and thus advanced tests are more improved by increased education than is oral maths, as could be expected.

Comparison of the estimated impacts between the different tests draws the following picture: knowledge on life skills is likely to be gained out of school but is not improved through work, knowledge on oral maths is gained through school but can be acquired through participation to economic activities (one year spent working allows the children to learn as much as half what they would have learned from school on that topic), while the discrepancy between what can be learnt at school and from work is higher for advanced (and written) tests. Both tests in mathematics being quite comparable, except for their modalities (oral vs. written), it is quite likely that the main difference between the estimates comes from the fact that less educated children are not able to read and write.

4.2 Controlling for social background variables

If the scores are insufficient to control for heterogeneity in abilities or other factors determining both human capital investments and results, then adding some social background variables may alter the results if these variables are able to pick up some heterogeneity. We now try to introduce such additional controls. It is also interesting *per se* since it will provide us with some estimations of the effect of social background on learning.

We thus run the same specification with the following controls: age (as previously), gender, father's and mother's education, wealth, detention of productive assets, household's size and whether the child has an older brother and an older sister. Results are given in Table 7.

Estimates for the effect of the number of years of schooling are very similar to what we found before without control variables; results change a bit for the effect of labor but not systematically in the same way. The most notable discrepancy between this set of result and the previous one is for the impact of child labor on written French but in this case, we do not reject the hypothesis that the coefficients of the new variables are jointly equal to zero. This is evidence for the fact that we ought to prefer the constrained model, which is more precisely estimated. Father's educational level and wealth tend to improve learning. Otherwise, the control variables are rarely significant, even at the 10% level, attesting that control for abilities also capture differences in social background. The table also shows that, as expected, the point estimate for age is negative.

4.3 Taking into account domestic work

The identification strategy we offer allows children's time allocation and knowledge acquisition to be correlated with abilities, communities' characteristics and social background variables. It does not prevent though from biases emerging from participation to domestic work. If children who perform economic work are

Table 7: Impact of child labor and schooling with controls (option 2)

	Life skills	Oral maths	Written maths	Written French
Years of schooling	0.101**	0.126**	0.201**	0.156**
	(0.021)	(0.022)	(0.026)	(0.028)
Years of labor	0.001	0.031	0.033+	0.055**
	(0.018)	(0.019)	(0.019)	(0.021)
Age	-0.075+	-0.043	-0.147**	-0.088+
	(0.041)	(0.044)	(0.046)	(0.049)
Boy	-0.114	0.172+	0.076	-0.138
	(0.090)	(0.094)	(0.092)	(0.100)
Father's education	-0.011	0.044	0.047+	0.045
	(0.027)	(0.028)	(0.027)	(0.030)
Mother's education	0.013	-0.004	-0.026	-0.007
	(0.038)	(0.039)	(0.038)	(0.041)
Wealth	0.132+	0.122+	0.143+	0.102
	(0.070)	(0.074)	(0.075)	(0.082)
Land area	0.001	-0.000	-0.009	-0.021
	(0.013)	(0.013)	(0.014)	(0.015)
Livestock	0.012	-0.038	0.019	-0.016
	(0.032)	(0.034)	(0.035)	(0.038)
Enterprize	0.110	0.099	0.068	0.020
	(0.089)	(0.093)	(0.093)	(0.102)
Household size	0.005	0.006	0.001	0.007
	(0.008)	(0.008)	(0.008)	(0.009)
No older brother	0.004	0.132	0.071	0.122
	(0.080)	(0.084)	(0.085)	(0.092)
No older sister	-0.225**	-0.127	-0.096	0.007
	(0.084)	(0.088)	(0.089)	(0.098)
Math Score in 95	0.023**	0.013	0.019*	0.023*
instrumented by French score	(0.009)	(0.009)	(0.010)	(0.011)
# Obs.	521	509	426	425
R^2	0.41	0.36	0.45	0.37
F test (p-value)	0.079	0.065	0.271	0.171

Note: Scores are normalized by their standard errors thus estimates are the impact of one additional year of labor on scores in 2003 measured in standard errors. Controls include communities fixed effects. **, * and + mean respectively that the coefficient is significantly different from 0 at the 1%, 5% and 10% level.

systematically less (more) put to contribution for domestic chores and that doing some housekeeping prevents from fully benefiting of the received education, then coefficient for child labor will be upwardly (resp. downwardly) biased. Domestic chores for the children include fetching wood and water, cooking, laundry and can take long hours. We thus ought not to assume that its effect is nil.

In order to assess this issue, we rely on the declaration of activities made by the children in 1995. We thus create a variable of participation to domestic tasks if the child declares doing at least one of the following activities: cooking, laundry, dishwashing, cleaning. Fetching wood and water is unfortunately excluded from the list but this activity is expected to be correlated with the others. Correlation between number of years spent working and participation to domestic activity is as high as -0.25: children who carry out one of the activities are less likely to be put to contribution for the other. In this case and if housekeeping is detrimental to learning then estimate of the impact of child labor is upwardly biased, which could explain our findings.

We thus include this new variable in the regressions. Table 16 in appendix demonstrates high standard errors for the estimates of the impact of domestic work, which is likely due to measurement error. As a consequence, impact of child labor is less precisely estimated. For the written mathematics, we fail to find any significant impact of child economic work. However, despite mourning over the loss of precision, we notice that the point estimates remain quite close to what was found before (when there is a change, it is a slight decrease, as expected). Given the imprecision, the point estimate for domestic work varies much more from one specification to the other than the other coefficients and manages to be significantly different from zero in only one of the terminal specifications. Let us comment the size of the estimate if we were to retain an estimate of 0.10 for instance. Unlike the definition of economic work which is counted in number of years, participation to domestic tasks is simply whether the child performed some when he was in second year of primary school. Assuming that a child who was participating at that age continues to do so until the date of the survey then this estimate corresponds to the impact of 7-8 years of participation. An increase in one year of domestic work would then lead to a decrease of 0.014 standard deviation in learning. Without taking these results at face value[17], we can note that in absolute value it is relatively small compared to the impact of economic work and schooling on learning and does not require to push the analysis further on.

Another strategy is to question the determinants of child's time allocation between the two activities. Namely, if the correlation between the two activities can be zero when controlling for some factors, then it is useless to control for participation to domestic activities *per se*. Without great surprise, conditioning by gender is sufficient to de-correlate economic and domestic labor. Gender was already included as an additional control in the previous section and we saw the results were robust to this inclusion. As a conclusion, taking domestic work into account does not alter the results, probably because the effect is quite small in this setting.

[17]The measurement error leads to an attenuation bias.

4.4 Summary

The different strategies find very similar results (different options for score controls, control for other characteristics and control for domestic work). When controlling for the time devoted to education, having worked during childhood does not hamper learning but the benefit it provides is quite small. This little gain could arise from increased resources due to the participation of the child to economic activities or from learning due to a higher parents' frequentation. By comparison, the effect of work is at best 1/3 the one for schooling and it is clear that if time allocated to work is taken on schooling time (meaning fewer years of education), work implies a lower cognitive achievement at the end of the day.

4.5 Including intensity at work and type of occupation

So far, we have implicitly assumed that all years of work were comparable; it is indeed not the case if some children perform economic activities only in evening or in the week-end while others do it more extensively. It is quite likely that the small benefit of work we have identified could vanish if work is performed during long spells. To put it differently, the impact of work could be heterogenous among workers depending on the intensity of their activity. The effect α of number of years spent working is decomposed as:

$$\alpha = \alpha_0 + \alpha_I \cdot \text{Intensity}.$$

This implies to re-estimate the model with intensity times number of years spent working as an additional variable:

$$T_{ic} = \alpha_0 W_{ic} + \alpha_I W_{ic} \cdot \text{Intensity}_{ic} + \beta S_{ic} + \gamma X_{ic} + \sum_c \delta_c + u_{ic}.$$

The control strategy is particularly useful at this stage since we just have to introduce this new variable in one of the agreed specifications. Given the previous results, we expect α_0 to be positive and we presume α_I may be negative.[18] As already said, we consider the number of hours spent working when the child started as a proxy for the intensity of work during childhood. The intensity measure is given in number of hours per week in order to be interpretable. This being said, this measure is likely to be spoilt with measurement errors and its estimate might be biased towards zero. For the sake of simplicity, we just provide the results when using the second strategy (as in column 7 of Table 5), given that the previous results were almost the same and that this estimation uses the largest number of observations.

[18] We also tried a model where α has a quadratic term in intensity in order to allow the effect of intensity to be positive and then negative but in this case, none of the estimates is significantly different from zero except α_0. When plotting the estimated effect of child labor as a function of its intensity, we see that it readily starts by decreasing and that the estimated concavity is small. We thus focus on the results when the heterogenous effect is only linear in intensity.

Table 8: Impact of work's intensity on results

Scores in 2003	Life skills	Oral maths	Written maths	Written French
Years of schooling	0.095**	0.118**	0.188**	0.150**
	(0.021)	(0.022)	(0.026)	(0.027)
Years of labor	-0.009	0.038+	0.062**	0.055*
	(0.019)	(0.021)	(0.022)	(0.023)
Intensity x Years of labor	-0.000	-0.000	-0.004**	-0.003+
	(0.001)	(0.001)	(0.001)	(0.001)
# obs.	530	518	434	433
R^2	0.38	0.31	0.43	0.38

Note: Estimations include communities fixed effects, control for maths score in 95, which is instrumented by French score in 95 and control for child's age. **, * and + mean respectively that the coefficient is significantly different from 0 at the 1%, 5% and 10% level.

Table 8 shows that, despite the imperfection of the measure, we do find a negative impact of long hours spent working for advanced tests; this is not the case for the easier test of oral mathematics. With this specification, it appears that the effect of an additional year of work is $0.0615 - 0.0035 \cdot \text{Intensity}$ in the case of written maths and $0.0553 - 0.0025 \cdot \text{Intensity}$. As a consequence, if the number of hours spent working remains the same throughout childhood, work has a detrimental effect when intensity of work exceeds 17 hours a week for written mathematics and 22 hours a week for French tests. 75% of children of the sample who have an activity work actually less than 15 hours per week; for most children then, the impact of participating to economic activities remain positive for a given amount of education. Who are the children for whom the activity is detrimental to their learning? Among those who work more than 15 hours a week, 59% of them help their family in their activity; while 13% of them are employees and 21% apprentices. Nevertheless, if we compare these figures to the ones for the population of children aged 13 to 18, it appears that employees and apprentices are more often among the children who work long hours while familial workers are more often among the ones who work a lower number of hours. As a consequence, employees and apprentices are more at risk of not fully benefitting from their education. We check this by running similar regressions but including the occupation status of the child when he started to work (Table 17) and employee shows up as highly detrimental to the learning process: it decreases by half a standard error results to tests.[19] While targeting

[19]The results should be taken with extreme caution since the number of children in each of the status categories are low (e.g. only 20 children in the sample are employed outside of the household).

efforts to reduce prevalence of work on the children who are employed outside of the household could be an option to decrease the harmful consequences of child labor, it would clearly not guarantee that other children are safe from such consequences since most of children who work during long spells are employed by their own family.

5 Conclusion

This paper offers an estimate of the impact of working during childhood on cognitive achievement. For a given amount of schooling and once heterogeneity in environment and abilities has been control for, children who have been working do not perform worse but even slightly better than the others. This could be either due to an increase in resources that allow the child to learn better if the resources are allocated to schooling inputs, or to knowledge gained from working with the parents. The positive impact of work vanishes though if the child worked more than 17 hours a week on average when he started his activity.

It would be of interest to check with other data whether the effect of performing domestic activities is really negligible on child's learning at school since it is the prevalent activity for girls in most developing countries.

6 Appendix

Table 9: Descriptive statistics for normalized tests scores

Test	# obs	Mean	Std Dev	Min	Max
Life skills	578	3.72	1	.87	5.81
Oral maths	565	2.95	1	0	4.80
Written maths	470	3.78	1	0	4.97
Written French	469	4.17	1	0	5.27

Table 10: Correlation between scores

	math in 95	math in 96	French in 95	French in 96
math in 95	1			
math in 96	0.40	1		
French in 95	0.39	0.39	1	
French in 96	0.32	0.61	0.45	1

Note: Correlations computed on the sample of children aged 13 to 18 and who have taken the tests. All the correlations are statistically significant at the 1% level.

Table 11: Description of control variables

Name	Description	Min	Max
Age	Age of the child in 2003	13	18
Boy	1 if a boy, 0 otherwise	0	1
Father's education	1, no education	1	8
or	2, uncomplete primary		
Mother's education	3, complete primary		
	... 8 university		
Wealth	indicator of permanent income built	-1.45	2.99
	on information on durable goods and housing		
	centered, reduced (see Table 12)		
Land area	Land area owned by the household	0	13.5
	measured in ln(sq.meters)		
	0 if the household does not own any land		
Livestock	indicator aggregating cattle, chicken...	-1.62	5.61
	centered, reduced		
Enterprize	1 if the household operates a firm, even informal,	0	1
	0 otherwise		
Household size	number of persons living in the household	3	30
No older brother	1 if the child has no older brother, 0 otherwise	0	1
No older sister	1 if the child has no older sister, 0 otherwise	0	1

Table 12: Wealth indicator: weights given to the variables

Durable goods		Housing	
Cooker	0.310	Walls in concrete	ref
Fridge, freezer	0.691	Walls in bricks	-0.140
Coal oven, “gaz butane”	0.441	Walls in earth	-0.356
Electric, gas, micro-wave oven	0.166	Walls in bamboo, canvas, other	-0.068
Sewing machine	0.260	Walls in wood, galvanized iron	0.017
Fan	0.688	Walls in stone	-0.204
Air conditioned	0.158	Floor in concrete, cement	ref
Radio	0.004	Floor in sand, earth, bamboo	-0.578
Tape, record player	0.398	Floor in wood	0.012
Television	0.775	Floor in stone, tile	0.445
Video cassette recorder	0.445	Ceiling in galvanized iron	ref
CD player	0.263	Ceiling in leaves, earth	-0.526
Camera, video	0.165	Ceiling in wood, canvas	0.043
Computer	0.185	Ceiling in concrete, cement	0.531
Bicycle	0.101	Ceiling in tile	0.244
Motorcycle, scooter	0.115	Windows poorly protected (c)	-0.557
Car	0.340	Multi-storied house or apartment	ref
Cable TV	0.472	One-story house	0.273
Electric iron	0.289	Apartment(s)	0.209
Furniture	0.369	Number of rooms (c)	0.139
		Well without pump	ref
		Inside private tap	0.525
		Water seller	0.019
		Outside private tap	0.282
		Well with pump	-0.107
		River, rain water, other	-0.059
		Public standpipe	-0.147
		Low hygienic toilets (c)	-0.725
		Electricity	0.814
		Kitchen	0.129
		Remote telephone (c)	-0.595

Note: (c) means that this variable take several ordered modalities.

Table 13: Number of observations

Test	Life skills	Oral maths	Written maths	Written French
Score in 2003 + Scores in 95	542	530	443	442
Score in 2003 + Scores in French (95-96)	492	-	-	405
Score in 2003 + Scores in Maths (95-96)	520	506	425	-

Note: Number of children aged between 13 and 18 and for which number of years of education, age when started to work are available.

Table 14: Descriptive statistics for the scores using different criteria

Criteria	# obs.	Mean	Std Dev	Min	Max
Life skills score					
All	2354	61.89	18.62	0	100
$13 \leq$ age ≤ 18	2321	61.97	18.56	0	100
+ has been to school	2040	63.44	18.21	0	100
+ both tests avail. in 95	578	63.98	17.19	15	100
+ both tests avail. in 96	517	64.36	16.95	15	100
Oral maths score					
All	2200	59.18	22.20	0	100
$13 \leq$ age ≤ 18	2177	59.19	22.19	0	100
+ has been to school	1923	60.84	21.68	0	100
+ both tests avail. in 95	565	61.51	20.82	0	100
+ both tests avail. in 96	506	61.51	20.59	0	100
Written maths score					
All	1641	76.32	20.42	0	100
$13 \leq$ age ≤ 18	1624	76.33	20.45	0	100
+ has been to school	1595	76.74	20.17	0	100
+ both tests avail. in 95	470	76.01	20.11	0	100
+ both tests avail. in 96	426	76.20	19.94	0	100
Written French score					
All	1703	78.52	19.89	0	100
$13 \leq$ age ≤ 18	1680	78.63	19.82	0	100
+ has been to school	1647	79.01	19.53	0	100
+ both tests avail. in 95	469	79.04	18.95	0	100
+ both tests avail. in 96	424	79.83	18.25	0	100

Table 15: Predicting attrition in sample

	Household reinterviewed	Child tested
Boy	0.252	0.222
Lives with one's father	−0.052	0.143
Lives with one's mother	0.219+	0.107
Lives with one's grand-parents	0.222*	−0.018
Wealth	−0.056+	0.055+
Rural	0.424**	−0.214
CI repeated	0.191	−0.163
CP repeated	−0.113	−0.285*
Helped with homework	0.243+	−0.049
Domestic work	0.193	0.139
Agricultural work	0.226*	0.107
Business activity	0.137	−0.012
French score in 95	0.003	−0.003
Math score in 95	−0.007+	−0.002
# Observations	982	790

Note: **, * and + mean respectively that the coefficient is significantly different from 0 at the 1%, 5% and 10% level.

Table 16: Including domestic work

Scores in 2003		1	2	3	4	5	6	7
Oral maths	Eco. work	-0.005	-0.004	0.025	0.027	0.033+	0.039*	0.028
		(0.013)	(0.013)	(0.017)	(0.017)	(0.018)	(0.019)	(0.018)
	Dom. work	-0.162+	-0.153+	-0.221**	-0.197*	0.047	-0.109	-0.188*
		(0.084)	(0.085)	(0.083)	(0.084)	(0.113)	(0.097)	(0.093)
Written maths	Eco. work	0.002	0.005	0.025	0.025	0.027	0.028	0.026
		(0.014)	(0.014)	(0.018)	(0.017)	(0.019)	(0.020)	(0.019)
	Dom. work	-0.176*	-0.165+	-0.184*	-0.171*	-0.048	-0.109	-0.142
		(0.086)	(0.086)	(0.084)	(0.083)	(0.115)	(0.098)	(0.090)
Written French	Eco. work	0.009	0.013	0.035+	0.034+	0.030+	0.034	0.033+
		(0.015)	(0.016)	(0.020)	(0.020)	(0.018)	(0.022)	(0.020)
	Dom. work	-0.005	0.018	0.002	0.014	0.107	0.042	0.026
		(0.093)	(0.093)	(0.093)	(0.093)	(0.107)	(0.107)	(0.095)
Fixed effects		no	no	yes	yes	no	yes	yes
Scores in 95		no	yes	no	yes			
Scores in 95 instrumented by 96						yes	yes	
Math scores in 95 instrumented by French score in 95								yes

Note: Scores are normalized by their standard errors thus estimates are the impact of one additional year of labor on scores in 2003 measured in standard errors. Control tests for specifications (2) and (4) to (6) are French in 95 for written French and mathematics for all the other ones. **, * and + mean respectively that the coefficient is significantly different from 0 at the 1%, 5% and 10% level.

Table 17: Impact of occupation in childhood on results

Scores in 2003	Life skills	Oral maths	Written maths	Written French
Years of schooling	0.094**	0.124**	0.197**	0.145**
	(0.022)	(0.023)	(0.026)	(0.028)
Years of labor	0.020	0.020	0.027	0.033
	(0.026)	(0.028)	(0.029)	(0.031)
Employee	-0.344	-0.106	-0.472+	-0.577*
	(0.236)	(0.248)	(0.272)	(0.287)
Self-employed	0.089	-0.108	0.070	0.392
	(0.277)	(0.290)	(0.347)	(0.367)
Familial worker	-0.254	0.123	0.037	-0.035
	(0.172)	(0.181)	(0.186)	(0.197)
Apprentice	-0.098	0.156	-0.343	-0.361
	(0.199)	(0.214)	(0.250)	(0.278)
No occupation	Ref	Ref	Ref	Ref
# obs.	542	530	443	442
R^2	0.38	0.31	0.42	0.36

Note: Estimations include communities fixed effects, control for maths score in 95, which is instrumented by French score in 95 and control for child's age. **, * and + mean respectively that the coefficient is significantly different from 0 at the 1%, 5% and 10% level.

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